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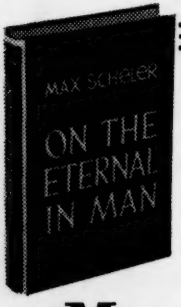

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THE JOURNAL OF PHILOSOPHY

MOORE'S APPEAL TO COMMON SENSE

IN "A Defence of Common Sense,"[1] G. E. Moore indicates five matters in which he differs from certain other philosophers. (1) He holds that the common-sense view of the world is in certain fundamental features wholly true. (2) He sees no good reason for believing *every* physical fact to be logically or causally dependent on some mental fact. (3) He admits no good rational proofs for the existence of God and the immortality of the soul. (4) Whereas he is convinced that the proposition "There are and have been material things" is certainly true, he is doubtful as to the proper analysis of such material object statements in terms of sense-data. And (5) whereas he is convinced that the proposition "There are and have been many Selves" is certainly true, he is doubtful as to the proper analysis of this proposition also. His conclusiveness in (2) stems from (1); his indecision in (3), (4), and (5) may also stem from (1) or else, as he allows,[2] from his philosophic method, or from both. In any case it is impossible to appreciate either Moore's conclusions or his problems apart from his appeal to common sense. The primary purpose of this article is to examine this appeal and to show that he has provided therein a philosophical starting point that merits serious consideration, rather than the indulgent smiles to which it has more often been treated.

It must not be thought that so influential a thinker as Moore was himself detached from the moulding influence of his philosophical training. His intellectual autobiography makes this plain.[3]

[1] *Contemporary British Philosophy*, 2nd Series, ed. by J. H. Muirhead (London, 1924), pp. 191–223.

[2] "I did want to answer questions, to give solutions to problems, and I think it is a just charge against me that I have been able to solve so few of the problems I wished to solve. I think probably the reason is partly sheer lack of ability and partly that I have not gone about the business of trying to solve them in the right way."—From "A Reply to My Critics," in *The Philosophy of G. E. Moore*, ed. by P. A. Schilpp (Evanston, 1942), p. 677.

[3] Schilpp, *op. cit.*, pp. 1–39. Cf. S. Z. Hasan, *Realism* (Cambridge, 1928), pp. 228–285.

One can also trace the influence of the introspective psychology of his day, and note intriguing parallels with the Scottish realists. Moore's appeals to common sense and psychological facts place him in a significant historical tradition. Our discussion will therefore suggest that it is anachronistic to regard him as just a linguistic analyst.[4]

We shall examine three emphases in Moore's common-sense argument (knowledge, belief, ordinary life), and then probe its implications for the problem of truth.

KNOWLEDGE

"The 'Common Sense view of the world' is, in certain fundamental features, *wholly* true."[5] But it is, of course, necessary to say what is meant by "certain fundamental features." Moore lists a number of truisms which he regards as certainly true and so obvious, in fact, as to be hardly worth stating. My body exists now, and has had a continuous history since its birth, in spatial relations to other historically existing physical objects, some of which are human bodies like my own. This much I know regarding myself, and very many other people have known parallel things regarding themselves.

Just as it cannot be disputed that these are truisms so it cannot be disputed that they are known. I simply know that I know. We can go no further than this, for knowledge is a simple, irreducible experience. It is a psychological fact that I *do* know, and nobody should deny this any more than other private experiences, whether emotions or pains or imaginary ideas. Of course, the sceptic can deny it if he chooses; but he has no reason at all for doing so, for he cannot penetrate into my private experience and tell me I do *not* know. It makes no difference that I may be unable to adduce logical support. I cannot do so for any private experience. It is simply a fact that needs no support. "The only proof that we do know external facts lies in the simple fact that we do know them. . . . We may quite well know many things which do not logically follow from anything else which we know."[6] Not all knowledge stands as the conclusion of syllogistic or dialectical reasoning. There are some things which we just know, and we

[4] This conclusion is similar to that of Alan R. White in Ch. I of his recent book, *G. E. Moore: A Critical Exposition* (Oxford, 1958). In Ch. IX he explicitly compares Moore with Thomas Reid.

[5] "Defence of Common Sense," p. 207.

[6] "Hume's Philosophy" (1909), *Philosophical Studies* (New York, 1922), pp. 160 f. Cf. "The Nature and Reality of Objects of Perception" (1905–6), *Philosophical Studies*, p. 96, *passim*.

have to admit it. This conviction is irresistible; it is overwhelming. We cannot help ourselves.

In *Some Main Problems in Philosophy*,[7] we find the same kind of argument, this time addressed to David Hume. Hume has laid down the principle that nobody can know the existence of anything except as a direct impression with force and vivacity, or else as an inference from the constant conjunction of such impressions. Unless these conditions are fulfilled one cannot know that physical things exist. But, Moore argues, since I *do* know that certain specific things do exist it follows by *modus tollens* that Hume's principle is not true. In fact, even by itself, it is much less certain than the proposition which it is supposed to prove false. For immediate knowledge cannot be readily disposed of, no matter what theories may be constructed deductively from other premises. Logical arguments cannot gainsay psychological facts. They must face them and take them into account. And our knowledge that specific physical objects exist is such an indisputable and irreducible fact.

Clearly, Moore leans towards a view common in British philosophy around the turn of the century. J. Cook Wilson, for instance, placed knowledge on a level with feeling and conation, and refused to inquire into its definition. For him there could be no theory of knowledge as such. Every inquiry pre-supposes that we already have some knowledge and that we already know what it means to know. H. A. Prichard followed Cook Wilson in regarding knowledge as a *sui generis* relation; and the idealistic view of knowledge was rejected on this basis. Now undoubtedly Moore is still concerned with the theory of knowledge. But it is important to notice that, while he is not averse to analyzing knowledge introspectively, he is averse to the demand that all knowledge must have logical support. Knowledge is fact, and facts need no justification. The problem of Hume and Kant is a pseudo-problem, and the idealistic argument is a pseudo-argument; for, whether or not it is regarded as philosophically significant, the fact remains that we know that we know physical objects. To disregard this is to engender contradictions. And this is precisely how philosophical paradoxes arise.

BELIEF

It seems that what Moore means by "know," others refer to as "believe." On page 206 of "A Defence of Common Sense," he speaks of his truisms as "common sense beliefs." But this does not mean that for him they are *only* beliefs, or even merely probable. ". . . it seems to me that I *do* know them, with certainty,"

[7] London, 1953, pp. 119, 143, *passim*.

whether or not any actual evidence can now be adduced. If, then, "belief" implies uncertainty, these are not beliefs. Even the term "probable knowledge" is distasteful, for, though we may not have demonstrable knowledge, it is by no means any the less certain.[8]

Nor does this mean that it is custom which justifies common-sense beliefs. Custom alone is sufficient neither to prove beliefs true nor to prove them false. It merely helps produce them. Some of them we do in fact know to be true, but some may be sheer prejudice.[9] The point to be emphasized is rather that belief is instinctive, that we find ourselves with "a strong propensity to believe."

> We believe that we are at certain times conscious of certain material objects: we see, and feel, and think of them. But we believe with the utmost certainty that these material objects can and do continue to exist even when we are *not* conscious of them.[10]

If, then, this is called belief, it must still be regarded as a form of knowledge. It is necessary, however, to distinguish the psychological question of the origin of beliefs, from the epistemological question of their justification. Some beliefs lack justification, so that while we know we believe certain things, while the belief may be intense and clear, yet at the same time this is no guarantee of truth. Moore is enough of an empiricist to say that belief is true when the thought we actually have before our minds corresponds to a fact. Truth is the relationship of which we are aware when we profess certainty.[11] In other words, there are some facts to which we can point, obvious facts which correspond to true beliefs. When we can point to such facts and such a relationship, we "believe with the utmost certainty." When we find we cannot, belief becomes a tentative committal.

This introduces the problem of error. We may *think* we have sufficient, obvious facts to justify a belief, which we accordingly call certain, whereas actually these facts may be inadequate or even irrelevant. In this case we are in error. Popular beliefs need to be modified and corrected over a period of time and as a result of scientific knowledge. But how, then, may the truth of a belief be guaranteed? A correspondence test can only produce degrees of probability proportionate to the available evidence. Even a causal

[8] "Reply to My Critics," p. 677. Miss Stebbing has expressed indebtedness to Moore for showing "that probable knowledge is no less *knowledge* than *demonstrative* knowledge," whereas Hume had assumed we have either demonstrative knowledge or mere belief (Schilpp, *op. cit.*, p. 526).

[9] "Hume's Philosophy," p. 161.

[10] *Main Problems*, p. 9.

[11] *Ibid.*, pp. 278 f.

inference theory would not help, for the process of causal inference is really one of inductive generalization, and this, too, cannot take us beyond probability.[12]

How, then, can Moore speak of the certainty of beliefs? Is it not contradictory to insist on probability on the one hand, and on the other to insist on certainty? Two possible answers could be made. First, it could be pointed out that probability applies to the justification of beliefs and is an epistemological notion, while certainty is more usually called "certitude," and applies when there are facts which we *do* know; it is a psychological matter. This is dangerous. For to separate belief from knowledge, epistemology from psychology, is to reject the very common-sense argument which he is building. His analysis of knowledge is a conscious attempt to avoid the jungle of epistemological uncertainties by appealing to the indisputable simplicity of psychological certainties. Either he must wholly disregard the epistemological problem and take it to be a pseudo-problem, as did Cook Wilson and Prichard, or else he must relate it to the psychological issue by explaining how probability justifies the assertion of certainty. To hold on to both sides without relating them is to take a common-sense position in one hand and a contrary philosophical position in the other. And it is this very thing which Moore so despises in a Hume or a Bradley. Now evidently he could not go as far as Cook Wilson and Prichard, without making epistemology a matter of psychological analysis. This he declines to do; his practice forbids it. Nor could he use probabilities to justify certainty without implying that some, at least, of our certainties are untrustworthy and hardly worth having. In this case he must explain just how far he goes with common sense, and why thus far. This he must do, not by establishing epistemological criteria, for this would destroy the ultimacy of knowing with certainty; he must do so by a further analysis of common sense and its relationship to philosophy.

A second answer is possible. It could be said that Moore has no one consistent doctrine, that whereas his major appeal is to psychological fact he sometimes toys with the epistemological problem. But in this case he is bowing to a psychological compulsion to try to justify what needs no justification and can have no complete proof. We have, therefore, two opposing psychological compulsions, one of which is irrational—to justify what need not and

[12] This is explicitly stated and developed in "Objects of Perception," pp. 49–67. Generalization is based on observations which must satisfy three conditions, which are, however, not sufficient to give certainty. These conditions include Hume's similarity and constant conjunction, with the addition that the things observed must have existed and been real.

cannot be justified—and the other of which he takes as supremely rational—to accept some immediate presentations as certainly true. How, then, can I discern between irrational and rational impulses? Or are they both irrational? In either case are they both to be trusted? The one tells me I do not yet know for sure, and the other tells me I do. Which one is right, and in which cases? Here, too, he must explain just how far he goes with common sense; here, again, we need that further analysis of common sense and its relationship to philosophy.

ORDINARY LIFE

Common sense is an appeal to the common factors of ordinary life, which cannot be contradicted without contradicting oneself. To say, for instance, that we cannot know about other objects than ourselves is self-contradictory. For it makes an assertion about a "we" of whose existence and about whom we are entitled to know nothing. This "we" is one of the things in question. The truisms of common sense are essential to the very act of philosophizing about the possibility of a real world. If it be granted that we know them to be features in the common-sense view, we cannot say they are not true, "since to say *we* know this, is to say they are true." The philosopher who speaks of *our* perceptions assumes that something other than himself and what he directly perceives is real. The philosopher who asserts that human knowledge is limited to one's own private states "is implying that his own knowledge is not so limited." He can only suggest that *if* there are others like himself, they will have limitations like his own. Otherwise he knows something he cannot know.[13]

Ultimately, then, such scepticism is self-contradictory because it gives "an utterly false account of what we do believe in ordinary life." To say that all I know is the orderly succession of my own sensations loses all its plausibility when it is applied to particular cases.[14]

Moore's classical example appears in his British Academy lecture, "Proof of an External World." He "proves" that two human hands exist.

> How? By holding up my two hands, and saying, as I make a certain gesture with the right hand, "Here is one hand," and adding, as I make a certain gesture with the left, "and here is another. . . ."
>
> But did I prove just now that two human hands were then in existence? I do want to insist that I did: that the proof which I gave was a perfectly

[13] "Hume's Philosophy," pp. 158 f.; cf. "Objects of Perception," pp. 31 f.; "Defence of Common Sense," pp. 206 f.

[14] *Main Problems*, pp. 135 ff.

rigorous one; and that it is perhaps impossible to give a better and more rigorous proof of anything whatever.[15]

This is the sort of proof, he pleads, that we all use constantly and take as absolutely conclusive. We prove there are three misprints on a page by pointing them out one at a time. Now, of course, Moore is still saying we just "know." But he professes to find logical rigor in the process. The premise ("Here is one hand, etc.") is distinct from the conclusion. The premise is indisputably true; anything else is absurd. "You might as well suggest that I do not know that I am now standing up and talking—that perhaps after all I'm not, and that it's not quite certain that I am." And the conclusion really does follow from the premises, for one hand here and now, plus one hand here and now, equals two hands here and now.

Of course, the premise is itself a material object statement, and Moore assumes we all know for certain that it is true. This was sure to arouse a storm of opposition, and he tried to anticipate it by accounting for philosophical dissatisfaction with common sense. All he actually does is to refuse epistemological justification for those psychological facts which he regards as ultimate. The demand for justification is both impossible and unnecessary on his definition of knowledge.[16] This, then, gets us no further in the analysis of common sense. He simply repeats that what we must accept in ordinary life must also be accepted in philosophy. Common sense brings certain demands which he feels the philosopher dare not ignore.

The same appeal to ordinary life characterizes an earlier essay opposing Joachim's theory of truth. If, while one is in Oxford and the other in Edinburgh, both of them "conceive and discuss precisely and numerically the same truth," it cannot be denied that this truth is objective. Otherwise their philosophical intercourse would be impossible. By the same token, unless other facts of knowledge are objective, everyday experience would be impossible. And being objective, they are plain for all to see and know.[17]

At times Moore appeals to the language as well as the life of the ordinary man. To be sure, he recognizes that it is no safe guide. It lacks, for instance, the means of referring to objects of sensation as distinct from the act of sensation, and employs for

[15] British Academy, *Proceedings*, Vol. XXV (1939), p. 295.

[16] *Ibid.*, p. 299. Cf. the criticism leveled by Alice Ambrose (Schilpp, *op. cit.*, pp. 399 ff.), John Wisdom (Schilpp, *op. cit.*, p. 434 *passim*), and A. C. Ewing ("Knowledge of Physical Objects," *Mind*, Vol. LII (1943), pp. 106 ff.).

[17] *Mind*, Vol. XVI (1907), pp. 299–335.

both the one ambiguous term "sensation."[18] But, all the same, his use of the word "know" follows ordinary rather than philosophical language, as if the former is closer to common sense and therefore more reliable. As Alice Ambrose puts it, how few hairs must be left on a head before it is "bald"? And how certain must we be before we "know"?[19] There is no technical answer to either question. It is a matter of usage. It must not be thought that he is arguing merely from custom, as if he regards ordinary language as the final court of appeal. He regards it rather as a reflection, an expression of common life, subject to whatever limitations the latter may bear, and related to philosophy in much the same way. Even Norman Malcolm recognizes this, when he argues[20] that Moore's weakness lies in his not seeing that philosophical paradoxes arise from the misuse of language, but contenting himself with supposing a blindness to facts. For Malcolm, Moore did not take the language game far enough. But for Moore, ordinary language simply reflects the psychological certainties of ordinary life and common sense, regardless of their epistemological justification. The philosopher faces the task of analyzing and sifting their pronouncements and implications.

PROBLEM OF TRUTH

Thus far we have seen that Moore's analysis of common sense appeals to two things: the fact that we just "know," and the fact that it is self-contradictory to deny this. We have observed that the demands of common sense are psychological rather than logical, and that its priority excludes any purely logical justification. But at the same time the philosopher must be critical enough to discern those "certain fundamental features" in which the common-sense view is "wholly true." In 1952 he made it clear that not all sensing is knowing, and even that "there is no common use of 'know' such that from the mere fact that I am seeing a person it follows that I am at that moment knowing him."[21] Consequently while *some* sensing is indisputably knowing, we are still faced with the problem of determining specifically in which cases this is so. How may this be done?

One point has emerged. If a given view can be shown to be clearly inconsistent with what we cannot help accepting in ordinary life, then Moore will reject it. But by the same token if common sense involves no view of a given problem, then no theory can ever

[18] "The Refutation of Idealism" (1903), *Philosophical Studies*, p. 19.
[19] *Op. cit.*, p. 415.
[20] Schilpp, *op. cit.*, pp. 345 ff. Cf. Alan White, *loc. cit.*
[21] *Main Problems*, p. 77, note 1.

be shown to be inconsistent with it. Common sense entails no theory regarding the nature of sense-perception and physical objects, but only regarding the fact that there are such. The plain man has no sense-datum theory. It is therefore at such points that common sense gives way to philosophy.[22] And significantly enough it is on the sense-datum problem that Moore is most confused, and recognizes it.

In the first chapter of *Some Main Problems,* common-sense views are defined as those "held now-a-days by almost everybody." He means to exclude beliefs outmoded by the advance of human knowledge, and to isolate the invariable core of indispensable facts, such as "there are in the universe *a great number* of material objects." He recognizes that even truisms have been denied or added to by some philosophers. But he insists that in such matters common sense cannot be wrong. Its views may be supplemented, they may be analyzed, in peripheral matters they may be amended, but at root they are indispensable to consistent life and thought.

Now it has been observed that Moore regards custom as merely the *source* of common-sense beliefs, and in no way sufficient to prove either their truth or their falsity. How, then, can he say that ordinary life is sufficient to rule on the truth or falsity of certain views? Evidently he needs to make a distinction between custom and ordinary life. The former is the source of beliefs, whether they be true or false; it is no infallible criterion. The latter is the source of our assured knowledge of certain facts, and provides an infallible criterion of truth. It can be said to produce "necessary beliefs," moulded by essential factors, while the former can be said to produce "customary beliefs" moulded by accidental factors, in addition to the necessary ones. Beliefs that are merely customary represent historical and cultural distortions of, or accretions to, those that are necessary. And it is the latter, "necessary beliefs" to which Moore appeals. These represent certain root needs and basic commitments which he insists no man can avoid whatever his place in history or culture, and whatever his philosophical position.

In these terms, the problem confronting Moore, as he relates psychological facts to the epistemological problem and common sense to philosophy, is one of distinguishing customary from neces-

[22] "Defence of Common Sense," pp. 220 f.; "The Nature of Sensible Appearances," *Aristotelian Society, Suppl. Vol. VI* (1926), p. 187. R. P. Hawes, *The Logic of Contemporary English Realism* (New York, 1923), pp. 42 ff., points out that, for the realist, "Idealism does not keep close enough to experience, daily life, and science," that "The *prima facie* appearances of reality, the so-called facts, as well as scientific results, are not ultimate as given, yet they are nevertheless indispensable to complete truth."

sary beliefs. For him, certain truisms fit unavoidably in the latter category while some other assertions fit in the former. Yet there remains a measure of uncertainty and haziness, a "twilight" in which it is extremely difficult to see whether a given belief is necessary or not. It is these "twilight beliefs" which cause difficulty, and which account for the many divergences among realists. The prize specimen in Moore's case concerns the status of sense-data. If we recognize such a "twilight," we imply that no logically conclusive criteria are available whereby we can distinguish between the necessary and the customary. And this would, in fact, appear to be the actual situation in which we find ourselves. Often, even the most painstaking analysis fails to discern clearly whether proposition X is indeed false. It may take many years of philosophical study to show us that it is self-contradictory. For there is no easy way out of the hard and endless work of philosophy. Analysis and common sense only provide a method, a general criterion, and a starting point.

It appears, therefore, that we have only one criterion for deciding which common-sense views are "wholly true." There are some things which we "know with certainty," which are distinguishable from other things which we might profess to know with equal certainty. They are distinguishable by the fact that to deny them is to contradict oneself.[23] It is these assured truths which provide the foundation of philosophy. If they have been denied at times, this is either because the questions have not been adequately analyzed, or else because contradiction has been welcomed as the mark of profundity, rather than rejected as the mark of a failure which even the ordinary man can readily detect. To Moore's position, then, only two alternatives remain: either we neglect analysis or we welcome contradiction. The first path is an impossible one that can only stifle thought and produce obscurantism. Berkeley, to use an example that Moore would favor, thought he was pursuing common sense, but his subjectivism resulted from the failure to analyze effectively the dictum, *esse* is *percipi*, in the light of ordinary life. The second path will lead to the scepticism of a Hume or to the idealism of a Hegel and a Bradley, or else to the type of irrationalism that has no time for any philosophical analysis at all, and which therefore merges back into the first path. The sceptic can only avoid self-contradiction if he practices the solipsism he has conceived; otherwise he cultivates

[23] Alan White (*op. cit.*, Ch. II) makes no distinction *within* common sense, akin to our customary and necessary beliefs. His five criteria for common-sense statements appear simply to explicate the one criterion of consistency, in connection with the notion of self-evidence.

a schizophrenia. The idealist may indeed contend that only finite reality is contradictory. But common sense, life, and facts argue against him.

If this is so, the evident conclusion is that the philosopher must, like Moore, isolate those necessary beliefs which provide him with a legitimate starting point. He does so by analyzing the meaning of common-sense ideas and judgments, so as to determine which may be refined and sustained consistently with both themselves and other unavoidable facts.

ARTHUR F. HOLMES

WHEATON COLLEGE, WHEATON, ILLINOIS

COMMENTS AND CRITICISM

DID HUME EVER READ BERKELEY?

Antony Flew revives the question which I raised in this JOURNAL when Richard Popkin flatly pronounced that, in his opinion, Hume never read Berkeley.[1] Flew is willing to concur with Popkin, but for a curiously opposite reason. Popkin regards Hume's sceptical empiricism as so much more influential and important historically than Berkeley's discussion of abstract ideas that he concludes "(1) Hume's discussion of abstract ideas in the *Treatise* hardly resembles Berkeley's, (2) it resembles Huet's or Ramsay's at least as much as it resembles Berkeley's, and (3) Hume does not make anything of the point."[2]

Flew "clinches the first of these claims" by showing that "Berkeley's actual discovery [abstract ideas function as signs] was thus far greater and more valuable than that which Hume credited to him."[3]

Now, logically, neither Popkin's admiration for Hume's unique scepticism nor Flew's tribute to Berkeley's great and valuable theory of signs has anything to do with the question of whether Hume ever read Berkeley. The important philosophical point for the historian of philosophy is that Locke, Berkeley, and Hume form a continuum of discussion from the 17th century to our own day concerning the function of abstract ideas in science, morals, metaphysics, and religion. Popkin and Flew might as well say that neither Berkeley nor Hume had ever read Locke because each made individual contributions to the problem of abstract general

[1] See Vol. LVI (June 4, 1959), pp. 535–545, and Vol. LVII (Jan. 19, 1961), pp. 50–51.

[2] *Loc. cit.*, p. 539.

[3] *Loc. cit.*, p. 51.

ideas in relation to concrete particular ones. Likewise, Karl Marx, Bradley, or Whitehead never had to read Hegel to be influenced by the idea of an Absolute with its own dialectic, and surely each of these three thinkers was able to make his own version of the idea of an absolute totality of history and experience.

It is simply a matter of historical curiosity and factual accuracy to know whether Hume ever read Berkeley, and the little research I have done at Hume's University of Edinburgh (where Berkeley's ideas were discussed in Hume's time) leaves this small question still open. Certainly neither Popkin nor Flew maintains that Hume never heard of Berkeley's philosophy, but each wishes to emphasize the individual merits of his favored philosopher. I never questioned this historical individuality, but wished to call attention to the historical continuity of their discussion of abstract ideas, a continuity as real as each philosopher's individuality.

D. W. Hamlyn, in his recent well documented history of the philosophy of perception,[4] shows how Hume continued and yet in his own way pushed Locke's and Berkeley's principles to a sceptical extreme (*in re* substance and causation). First, in distinguishing "ideas" from "impressions" Hume thought he was restoring "the word idea to its original sense, from which Locke had perverted it, in making it stand for all our perceptions" (*Treatise,* I, i, 1, p. 2, n. 1). Apart from the three references of Hume to Berkeley (Popkin has not made it clear why Hume *ever* should have mentioned Berkeley if he had not read or taken some of Berkeley's ideas seriously), Hume agreed with Berkeley that all sensory qualities are on the same immediate footing as pain and pleasure, thus breaking down the mechanical division of primary and secondary qualities (*Treatise,* I, iv, 5, p. 236). This second reference to Hume's *Treatise* is given by Hamlyn along with others as evidence that "Hume follows Berkeley" in some specific points; for examples, in Hume's account of how belief or judgment enters into perception of size and distance (Hamlyn, p. 121), and in Hume's theory of a mathematical point as a *minimum visible* (*ibid.*, p. 123). Hume's scepticism certainly distinguishes him markedly from Berkeley's subjective idealism, and Hamlyn suggests the influence of Bayle's Dictionary article on "Zeno" as well as of Leibniz (*ibid.*, p. 124). Surely Hume could favorably regard (as he did in his *Abstract*) the idea of Leibniz on the desirability of including Probability in the study of logic without agreeing with Leibniz's extreme rationalism. He might have not even read Leibniz though

[4] D. W. Hamlyn, *Sensation and Perception: A History of the Philosophy of Perception* (International Library of Philosophy and Scientific Method, ed. A. J. Ayer), New York, 1961, pp. 116–124.

he mentioned his name. What is philosophically and historically important is that Hume had a glimpse of the idea of a logic of probable inference which Leibniz first broached as an extension of the old logic.

PHILIP P. WIENER

CITY UNIVERSITY, NEW YORK CITY

BOOK REVIEW

Gödel's Proof. ERNEST NAGEL AND JAMES R. NEWMAN. New York: New York University Press, 1958. ix, 118 pp. Cloth, $2.95; Paper, $1.75.

It is a good idea to try to expound the basic ideas of modern logic in such a way that the non-specialist can understand them. Russell did this in *Introduction to Mathematical Philosophy*, and produced a classic. Since then, logic has changed about as much as it did between Aristotle and Russell, and it is time for a new book of the same kind. Further, if such a book is to be written, Gödel's theorem is a good peg to hang it on. In fact I don't know a better. So the objective of Nagel and Newman is quite certainly valuable; but their execution of it is not.

The first three chapters are concerned with the historical background: the axiomatic method from Euclid to Hilbert. It is replete with strange, outdated perspectives, technical errors, and remarks which, to put it mildly, are misleading. E.g., in the discussion of non-Euclidean geometry (pp. 10–11) the following sentence occurs: "Immensely interesting and fruitful results are obtained when Euclid's parallel axiom is replaced by the assumption that more than one parallel can be drawn to a given line through a point, or alternatively by the assumption that no parallels can be drawn." This will certainly give the reader the impression that the two replacements are on a par, i.e., that one of them gives Lobachevskian geometry and one of them gives Riemannian geometry—whereas actually one of them gives Lobachevskian geometry and one of them gives a contradiction. This is part of the folklore of geometry, and it is depressing to find it here. Nor is one's appetite to continue reading stimulated by "the overall conclusion . . . that mathematics is simply the discipline that draws the conclusions logically implied by any given set of axioms or postulates" (p. 11). This is absurdly out of place in a book whose central topic is a theorem in the metamathematics of *number-theory*. The lay reader will quite certainly conclude

(and who can blame him) that arithmetic as well as geometry consists of exploring the consequences of a certain set of axioms. (If it were, Gödel's theorem would not be devastating at all. Nobody is disturbed over the fact that some groups are Abelian and others not, or any other incompleteness result in *hypothetical* mathematics.) The whole crucial distinction between hypothetical and absolute mathematics is glossed over completely in this book. It is only the incompleteness (or rather incompletability) of a branch of *absolute* mathematics, namely arithmetic, that is disturbing. To go on record once and for all, let me say bluntly: Arithmetic is not about the consequences of a set of axioms (as group theory or topology is). It is about numbers. And I know of no reputable logician (except possibly Curry) who thinks otherwise.

The purpose of the authors' beginning with non-Euclidean geometry is twofold: firstly, to indicate why modern mathematics is "more abstract and more formal" (p. 11) than traditional mathematics; and secondly, to motivate a discussion of consistency and consistency proofs and therewith of the Hilbert program which provided the historical background of Gödel's discovery. The entire discussion of consistency and the Hilbert program (Chs. I–III of the book) is misleading in a variety of ways. Firstly, because though the existence of consistent non-Euclidean geometries may have shown that *some* parts of mathematics (namely non-Euclidean geometry!) are abstract (in the sense of *being amenable to deductive treatment quite independently of considerations of subject-matter*), it did not do this for the two most important branches of mathematics (namely analysis, including Euclidean geometry, and the theory of numbers, otherwise known as arithmetic). Secondly, because the existence of consistent non-Euclidean geometries does not imply that mathematics (*even* hypothetical mathematics) is formal in the sense that "[*its*] *validity . . . is grounded in the structure of mathematical statements* rather than in the nature of a particular subject-matter" (p. 12; my italics). Surely there is a third interpretation of the validity of mathematical statements besides the Cartesian position that it is grounded in the properties of the subject-matter and the formalistic position (Curry's, not Hilbert's) that validity *means* manipulation of expressions in accordance with certain rules; namely, the interpretation according to which there are certain *a priori* truths (whether analytic or synthetic does not matter in the present connection) of the form "If any structure exists with the properties P, Q, R, . . . (e.g., with the properties involved in the notion of being a topological space), then necessarily it has also the property

S." That such statements constitute the *corpus* of hypothetical mathematics (of which algebra and topology are the two great subdivisions) is surely the most commonsensical account of the nature of such mathematics that can be given. It commits us to the position that hypothetical mathematics is abstract (which means simply that hypothetical mathematics consists of hypothetical statements), but *not at all* to the *further* position that hypothetical mathematics (still less mathematics generally) is formal, i.e., the position that the sentence last quoted is true *because* S can be obtained from P, Q, R, . . . by means of such-and-such formal manipulations. (This is brought out by the fact that we can meaningfully ask whether the kind of manipulations allowed are enough to permit *all* valid inferences from P, Q, R, . . . to be made. If the "formal" nature of hypothetical mathematics were as obvious as the authors seem to believe, it would be equally obvious that truth of such an implication would *mean* deducibility of the consequent from the antecedent. And that the deducibility relation coincides with the consequence-relation is false in some domains, and in others is an unobvious fact that requires proof.)

Thirdly, the authors' account of consistency and the Hilbert program completely overlooks the following simple fact. It is absurd that all of mathematics should be hypothetical. Even if we are completely formalistic in the sense that we regard correct mathematics as consisting of deductions carried out according to arbitrary rules from arbitrary axioms, we must admit the statement *that* such and such rules applied to such and such axioms do or do not yield such a conclusion—in particular, do or do not yield a self-contradictory conclusion—as a meaningful statement, true or false in itself and not *itself* a consequence of a set of axioms; just as, even though the positions of the men on a chessboard do not assert anything, the statement *that* white can mate in three is a perfectly meaningful one. The difference between the mathematical situation and the chess situation is this: the statement that a given mathematical axiom-system is consistent is *itself* a mathematical statement, whereas the statement that white can mate in three is not itself a configuration of chessmen. Nor is this point mere pedantry: it was precisely such considerations as this which led Hilbert to formulate his distinction between real and ideal statements. Most of (absolute) mathematics according to Hilbert consists of 'ideal' statements of which no objective correlate need be assumed to exist; the content of mathematics lies in its 'real' statements. In particular, the statement that a particular (absolute or hypothetical) axiom-system is consistent is a real statement; if such a system is inconsistent (relative to specified rules of in-

ference) the deducibility of a contradiction from it is an objective fact, not *itself* to be settled postulationally. To oversimplify a bit, the Hilbert program was designed to show that the use of ideal statements could not lead to any false real statements; if this could be proved, it would follow that in particular such use could not lead to any contradictions. This whole distinction of real and ideal statements, quite fundamental to the Hilbert program, is completely overlooked in the book under review, and it could not but be overlooked by authors who hold that *all* mathematics is hypothetical (and then go on to the further invalid conclusion that being hypothetical it must be formal).

The fourth and most serious objection to the authors' treatment of consistency is that they speak from a point of view which still takes the Hilbert program seriously. *The preoccupation with the consistency of absolute mathematics which characterized the later logical work of Hilbert is a thing of the past.* Only two major logicians, Kleene and Kreisel (the latter with significant differences), take such things seriously any more. The first three chapters of the Nagel-Newman volume regard the Hilbert program as still a live issue. Suggestive and historically important as it may have been, it is now moribund. (The consistency of *hypothetical* mathematics is of course still important in particular cases. However 'consistency' is here used chiefly in the sense of possessing a model rather than in the sense of not leading to a formal contradiction; and the appropriate techniques are mathematical rather than metamathematical.)

I now summarize the contents of Chs. II–III. Ch. II introduces the notion of consistency and gives some examples of proving consistency by means of models. The authors observe correctly that if the model is infinite, then consistency is proved only relative to the consistency of the model. (For example, the model method can be used to prove the consistency of some non-Euclidean geometries relative to Euclidean geometry, and of Euclidean geometry relative to high-school algebra. The consistency of high-school algebra may in turn perhaps be reduced to that of some still more pellucid model; but if an infinite regress is to be avoided, the process must stop with some theory whose consistency is either regarded as self-evident or established by means of a metamathematical argument.) The chapter concludes with a wholly inappropriate reference to the Russell paradox. This paradox is actually very closely related indeed to Gödel's theorem on account of its diagonal feature; however, the authors do not seem to know this. In any case, their reference to it in the present context is wholly inappropriate because the fact that the inconsistency of

naive (Frege) set-theory was not immediately apparent occasions only momentary qualms concerning the consistency of high-school algebra. The latter had been used for centuries with no trouble; Frege set-theory was a bizarre construction which had at the time of its appearance no mathematical antecedent. Oddly enough, the fact that high-school algebra *was* proved consistent by Tarski, using (metamathematical) methods presumably acceptable to Hilbert, is nowhere mentioned. Thus the reader is likely to be left to the end of the book with the picture of high-school algebra as an example of a part of mathematics to prove the consistency of which is a crucial, unattained, and apparently unattainable desideratum of metamathematics.

In Ch. III the notion of an absolute proof of consistency is introduced. (Recall, the central problem is supposed to be consistency, *sc.*, of actual mathematics. Ch. II claims to have shown that relative consistency proofs by means of models are not enough.) Absolute proofs of consistency are said to consist in the formalization of a theory (its exhibition as a set of strings of 'meaningless marks' and of operations leading from these strings to other strings) and in the demonstration by 'finitary means' that these operations applied to these strings can never yield 'self-contradictory' strings, e.g., $0 = 1$. All this is good as a partial exposition of the Hilbert program. I comment only (1) again, that almost no one takes the Hilbert program seriously any more, and so far we have had 36 pages about it; (2) that the obvious point is nowhere made that *any* proof of consistency is relative, in the sense that its cogency presupposes the correctness and *a fortiori* the consistency of *the methods used* in the consistency proof; (3) that we have not escaped the problem of an infinite domain, since these methods, in the case of Hilbert-type metamathematical consistency proofs, involve appeal to an infinite domain of *expressions;* (4) that the rather crucial fact that nobody has ever been able to give a clear account of what Hilbert meant by 'finitary means' is only alluded to casually in a footnote (p. 34); (5) that it is difficult or impossible to reconcile the authors' statement that mathematics is meaningless and metamathematics meaningful (pp. 28–30) with their later statement (pp. 76–82) that statements about a certain calculus can be "adequately mirrored" within the calculus itself. It is hard to see what this relation of "adequate mirroring" is; naively one would suppose it means "translating," but how can a meaningless (mathematical) string be a translation of a meaningful (metamathematical) statement? (Nor will the trick of calling it a quasi-translation, i.e., a string which is *provable* if and only if the original statement is *true,* suffice; the whole point of

Gödel's construction is to obtain a string which is provable if and only if the metamathematical statement which it "adequately mirrors" is *not* true.) Of course the relation of mirroring which exists between a metamathematical statement and its arithmetized counterpart *is* simply translation and there is no problem, because arithmetical statements, for all the authors say, are *not* meaningless; the authors are forced into the contradiction that a meaningless string can "adequately mirror" a meaningful statement by their odd position that all mathematics is hypothetical and/or meaningless. (Incidentally are these not mutually exclusive alternatives; how can it be both?)

Chapter IV tries to explain on eight pages two entirely distinct topics neither of which could possibly be explained to the lay reader in less than three times the space. One is *the formalization of the rules of logic used in mathematics* and the other is *the development of mathematics as a part of logic.* So condensed is the discussion that it is dubious indeed whether any reader without more than a semester's logical training will perceive that two entirely different subjects are being treated, that the senses of the word "logic" in the two phrases italicized above are radically different, and that the topic of one of the two discussions is entirely irrelevant to any of the preceding or following material. (Erratum: p. 40, footnote 1, "greater than" should be twice replaced by "equal to.")

Chapter V gives in outline a proof of the consistency of the propositional calculus. This is to serve as an example of an "absolute" consistency proof (the kind of consistency proof which is allegedly according to Gödel's theorem impossible for classical arithmetic). We must stress that the consistency of arithmetic *is* provable by a perfectly valid proof (even if not by one acceptable to Hilbert), specifically, by a truth-definition; and that all that Gödel's second theorem tells us is that the consistency of arithmetic cannot be proved *by certain means* (which means, however, are indeed adequate for proving the consistency of the propositional calculus). The reader will carry away the impression that there is a fundamental difference between theories like the propositional calculus, whose consistency can be "satisfactorily" proved, and theories like arithmetic for which this is not the case. This is only true if with the Hilbertian formalists we claim an absolute idea of wherein lies the "satisfactoriness" of a proposed consistency proof. (Recall, too, that the Hilbertian criterion of satisfactoriness is so subjective that there is no agreement as to whether Gödel's theorem precludes the possibility of a consistency proof which satisfies this criterion, nor more specifically as to whether or not the particular consistency proof offered by Gentzen is such a proof.)

The true situation about consistency proofs is this: Different (consistent) systems need tools of different strengths to prove their consistency. Usually, the stronger the system, the stronger the means required to prove it consistent. What kind of means are considered "acceptable" in consistency proofs depends on a variety of pragmatic considerations; the only scientific procedure is to study the properties of the relation "The proposition that System A is consistent can be proved in System B." This is the procedure of Feferman, Kemeny, MacNaughton, Wang, and even (in essence) of the neo-Hilbertian Kreisel. No hint of this whole approach, which has now replaced that of Hilbert, is to be found in the book of Nagel and Newman.

Chapter VI ("The Idea of Mapping and Its Use in Mathematics") is designed to motivate the introduction of Gödel-numbers. In view of this, the contents are as strange as the title. They include a discussion of the Richard paradox and of the duality theorem in projective geometry. I suspect that the authors' thought moved as follows. Gödel's argument rests on an isomorphism (!?) between the expressions of a formal system and the natural (i.e., non-negative whole) numbers. To explain it we need the idea of an isomorphism and an isomorphism is a kind of a mapping. Further than this I do not presume to trace the path of the authors' free associations. In any case, this chapter, like most of what precedes it, can be omitted without interruption of continuity.

The assignment of Gödel-numbers is an almost childishly simple device whereby we can translate any statement about formulas into a statement about numbers. It works like this. We invent a code whereby every string of signs in some given alphabet is translated into a number. For instance in the case of the English alphabet we could replace 'a' by "11', 'b' by '12', etc., down to 'z' by '36' and a blank space by '37'. Let us apply this procedure for illustration to the phrase 'we invent a code'.

w	e		i	n	v	e	n	t		a		c	o	d	e
33	15	37	19	24	32	15	24	30	37	11	37	13	25	14	15

The Gödel-number of 'we invent a code' is 33,153,719,243,215, 243,037,113,713,251,415. Now to every statement about formulas or other expressions (i.e., about strings of letters from the alphabet) corresponds another statement about the Gödel-numbers of those strings. For example the statement that ' we invent a code' is a sentence is true if and only if the large number that we have just written down belongs to a certain set S of numbers—namely, the set of Gödel-numbers of (English) sentences. All this is so very simple that its point is likely to be missed. What is not so

simple (but still within the powers of such a semi-popular book as this to explain) is that a great many of the most important properties of formulas (for example the property of being provable from Peano's axioms) correspond, under this coding device, to 'elementary' properties of natural numbers. ('*n* is the Gödel-number of a theorem' is an 'elementary' property of the number *n* in the technical sense that it can be expressed using only addition, multiplication, and such notions of elementary logic as 'and', 'not', 'all'.)

Gödel-numbering is no more mysterious than this. The vague ruminations on isomorphism which occupy Ch. VI will convince the untutored reader that it is something entirely beyond his grasp. His bewilderment will reach its climax in the central chapter of the book, Ch. VII, which deals with "Gödel's Proofs." Note the plural: there are *two* Gödel theorems, one to the effect that given any veridical axiomatization of number-theory we can effectively find a true formula not provable from that axiomatization; the other saying that arithmetic cannot prove its own consistency. The latter is the only one to which the earlier motivational material (pp. 3–56) has any relevance; however, its discussion occupies only two of the thirty pages of Ch. VII. The reader will have become quite sure by the time he reaches these two pages that because he has failed to see a relevance (of pp. 3–56 to the first twenty-eight pages of Ch. VII) which in fact does not exist, therefore he is doomed never to understand the profundities of Gödel's argument. What is called by most contemporary logicians 'Gödel's Theorem' is the theorem asserting the incompletability of arithmetic; the second theorem about the impossibility of formalizing a consistency proof for arithmetic within arithmetic itself is nowadays of highly specialized interest; most of this specialized interest, incidentally, consists of clarifying the *meaning* of the highly ambiguous phrase 'arithmetic cannot prove its own consistency', and it can truthfully be said that not until the relatively recent work of Solomon Feferman did anyone have a clear idea of just what Gödel *had* proved in his 'second theorem'.

Apart from the above, my criticisms of Ch. VII are two. Firstly, the system of Gödel-numbering is not the simple decimal coding (Quine's) which I expounded a couple of paragraphs ago, but Gödel's less transparent prime-power representation, in which for example the word 'code' would not be assigned the number 13, 251,415 as in Quine's coding, but instead $2^{13} \times 3^{25} \times 7^{14} \times 11^{15}$. The difference, mathematically insignificant though it is, is for the lay reader the difference between a smooth-flowing one-paragraph exposition and fourteen pages of turgid and irrelevant stumbling-

blocks. My second criticism is that, since Gödel, really crucial simplifications have been found in Gödel's argument, chiefly by Raymond Smullyan (though the basic ideas behind the simplifications are due to Post; Kalmar, Rosenbloom, and the reviewer also played a part). Not only has the essentially very simple central diagonal argument been thoroughly separated from the tedious combinational details (the 46 preliminary definitions referred to by Nagel and Newman on p. 68)—not only has the wood been thoroughly separated from the trees, but the trees themselves have been practically eliminated. In other words the *whole* of Gödel's argument (not just a simplified abridgment of which he cannot supply the details) can now, as a result of Smullyan's work, be presented to the lay reader in a most lucid way, with nothing missing. For all this see Smullyan's paper in the *Journal of Symbolic Logic*, Vol. 22, pp. 55–62; and await impatiently the publication in the Princeton *Annals of Mathematics Studies* series of his *Theory of Formal Systems*.

Nevertheless, the last chapter, on the philosophical significance of Gödel's result, is well-written, lucid, and to the point. That there are limitations to the axiomatic method, but that these limits can continually be pushed forward so that there is no definite point at which one can say "Thus far and no further" is indeed, as the authors maintain, the principal philosophical lesson of Gödel's (first) theorem. And I cheerfully plead guilty to all that they charge me with in interpreting it otherwise. This last chapter has a modern ring, and is a far cry indeed from the antiquated Hilbertianism of the first five. But why, having really understood the essence of Gödel's first theorem in their last few pages (the word 'consistency' appears only once in the entire chapter!) the authors did not see that the entire motivation of the earlier part of the book was miles beside the point, and thereupon delete the 41 pages (8–36 and 45–56) which deal with the Hilbert program and with consistency, or at least replace them by a two-page historical remark, is entirely beyond me to say.

An appendix deals with the Peano axioms (the original set, in terms of "successor," not Hilbert's version—I wonder how many readers will escape the false impression that Gödel's theorem applies to these axioms?), the use of the propositional and functional calculi in mathematical reasoning, and a subtle point about "internal" and "external" consistency proof concerning which, it is held, the reader may feel some confusion. The authors miss completely the point that *any* "external" proof of consistency (using a model) can trivially be transformed into an "internal" proof (using properties of formulas) by the simple device of re-

ferring to that property which a formula possesses if any only if it holds in the model. What was important to the Hilbertian formalists was not a distinction between 'internal' and 'external' properties of formulas (which seems as hard to make precise as the pseudo-scholastic one between intrinsic and relational properties) but between 'finitary' and 'non-finitary' properties and methods (which has also never been made precise, but which is at least on a vastly more sophisticated level). It is hard to see any respect in which the reader's confusion could be *lessened* by reading this portion of the Appendix.

My final verdict: the composition of a popular exposition of a technical subject is not a task to be scorned, nor undertaken by the inadequately prepared. Prime requisites for its satisfactory accomplishment are, first, an absolute sureness and authority on matters of detail; second, a lively awareness of the actualities of current science. In both of these regards, but more particularly and less forgivably in the second, the authors have fallen wretchedly below the standards upon which their innocent audience has a sacred but unenforceable right to insist.

JOHN MYHILL

STANFORD UNIVERSITY

COMMUNICATION

To the Editors of *The Journal of Philosophy:*

Professor John Myhill's review of *Gödel's Proof* is so absurd in content and so intemperate in manner that silence is perhaps the only response it deserves. However, since the editors of the JOURNAL deem the review fit for publication, our silence might be construed even by judicious readers as a tacit admission that Mr. Myhill's condemnation of our book is unanswerably just. We therefore ask for the opportunity to present a brief rejoinder.

The opening paragraph of Mr. Myhill's tirade (which runs approximately one fifth of our essay) suffices to show that he has not read our book with the care one expects of a responsible reviewer. He apparently thinks that our intent was "to expound the basic ideas of modern [Mr. Myhill understands by this the period since the publication of Gödel's 1931 paper] logic" for the non-specialist, although our Introduction explicitly states that "It is the aim of the present essay to make the substance of Gödel's findings and the general character of his proof accessible to the non-specialist."

Accordingly, his adverse comments on the ground that we fail to mention in our essay important developments in logic more recent than Gödel's paper, and that we make no use in our exposition of more elegant but also more recent techniques than those Gödel originally employed, are irrelevant. His "verdict" that since we lack "lively awareness of the actualities of current science" (an allegation apparently based on the supposition that our not mentioning various advances in logic since 1931 establishes our ignorance of them) we have "fallen wretchedly below the standards upon which [our] innocent audience has a . . . right to insist," is simply gratuitous impertinence.

It would be a hopeless task to discuss in short compass all the items in the long parade of sins of omission and commission which Mr. Myhill ascribes to us. A single example of the quality of his criticism on technical matters may be permitted. He asserts that our account of the Hilbert program "completely overlooks the . . . simple fact" that one "must admit the statement *that* such and such rules applied to such and such axioms do or do not yield such and such a conclusion . . . as a meaningful statement, true or false in itself and not *itself* a consequence of a set of axioms." But on page 35 of our book, for example, where we compare a formalized arithmetical calculus with chess, we expressly say that "although configurations of pieces on the [chess] board, like the formulas of the calculus, are 'meaningless', statements about these configurations, like meta-mathematical statements about formulas, are quite meaningful"; and the chapter in which this observation occurs, as well as the rest of the book, makes it clear that such meta-mathematical statements are *not* asserted as consequences derived from some axioms.

Although Mr. Myhill condemns us for our blunders when we comment on broad issues in the philosophy of mathematics, our shortcomings, as it turns out, consist in our failure to subscribe to his own views on admittedly mooted questions. We cite his cavalier dismissal, as no longer meriting serious consideration, of possible alternatives to his own near-Platonic conception of an "absolute" arithmetic; his parochial assessment of the Hilbert program as " a thing of the past" and "now moribund" (although K. Schütte's recent *Beweistheorie* will quickly disabuse a reader of the adequacy of Mr. Myhill's estimate of the extent of current concern with the problem of consistency); or his disingenuous suggestion that anyone who imagines there is some connection between the structure of Gödel's argument, the resolution of the Richard Paradox, and the notion of mapping, could have arrived at that conclusion only by way of a free association of ideas (although

such presumably "reputable logicians" as Gödel and Mostowski have explicitly noted such a connection).

We conclude with one additional instance of Mr. Myhill's shamefully irresponsible handling of our book. He offers as an erratum to the footnote on page 40 of our essay the following: ". . . 'greater than' should be twice replaced by 'equal to.'" However, the relevant portion of this footnote reads: ". . . 5 is greater than 3; therefore the square of 5 is greater than the square of 3." We could show without difficulty that all the other faults Mr. Myhill has so gleefully unearthed in our book are no graver than this one.

ERNEST NAGEL

COLUMBIA UNIVERSITY

JAMES R. NEWMAN

CHEVY CHASE, MARYLAND

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NOTES AND NEWS

Professor Philip Merlan, of Scripps College, Claremont, California, has received a Fulbright award to lecture in philosophy at the University of Oxford in the second and third quarters of the 1961–62 academic year, beginning January, 1962.

A Larrabee Philosophy Prize, named for Harold A. Larrabee, Ichabod Spencer Professor of Philosophy, emeritus, has been established at Union College through a gift from William R. Adams, Class of 1928. The prize, amounting to $75, will be given each year for the best work in philosophy by an undergraduate in the college, without regard to the class or major of the winner.

Studies in Romanticism, a new quarterly journal sponsored by the Graduate School of Boston University, will begin publication in the fall of 1961. The journal will be devoted to all aspects and manifestations of the Romantic Movement. The editor is David Bonnell Green.

Manuscripts dealing with any phase of the Romantic Movement will be welcome. The editor and sponsors would like to receive, among others, manuscripts on philosophy during the Romantic Period, on the sources and influence of that philosophy, and on the relationship of philosophy and other disciplines, such as music, economic history, and history of science.

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